

Interactive comment on “Causes of variation in soil carbon predictions from CMIP5 Earth system models and comparison with observations” by K. E. O. Todd-Brown et al.

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The paper by Todd-Brown et al. is an interesting study aiming to evaluate the C stock of the ESMs against data. The authors used simulations from CMIP5 using climate/carbon models and used two databases. The amazing stock of C stored in soils need to be better understood and represented in the models and in particular in the ESMs because few changes in the decomposition rate could lead to very important emissions. The paper is generally well written and the methods used are adapted. I think that this paper fits well with the BG scopes and is interesting for several scientific communities but some points must be discussed in more details before publication.

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I was a bit disappointed by the discussion section. The paper shows clearly that the ESMs could be improved in particular in the spatial distribution of C. But the discussion does not really suggest ideas or theories that must be incorporated in the next ESMs generation. I suggest the authors to look in details the Schmidt et al. (2011) review in Nature and write another paragraph with a more mechanistic approach. For example, do we need more biology in the models? or a better representation of the soil C profiles?

The authors also explained that all the ESMs are based on the same main scheme for soil C decomposition and the others existing schemes and how they could be used in this context are also not discussed (see Wutzler and Reichstein, 2008 or Manzoni & Porporato 2009 for review). The huge diversity of the existing soil C dynamic models must be presented briefly and the interest (or the absence of interest) of these new schemes in ESMs must be also discussed.

The authors considered that the soil C stocks calculated by the models represent the first meter but the model CENTURY where the main schemes of the ESMs soil modules came from was designed to simulate the SOC dynamics at a 20cm depth (Kelly et al., 1997). It is probably a problem if the data are re-analyze with only the first 20cm. The authors must justify better why they choose the first meter or reanalyze the data with the soil C stored in the 0-20cm layers.

Another point that must be clearly presented is the use of ESMs with specific climate for each model. It could be one of the main causes of differences between models. Indeed a ‘warm model’ is assumed to have less C in soils than a ‘cool model’. The study would have more sense with simulations of only the land surface models forced by the same climate. This must be clearly presented and the differences between the soil moisture and the soil temperature between the models must be presented.

Finally, I do not really understand the interest of the reduced complexity models. This approach lead to reduce the differences between models to simple parameters that

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are almost impossible to evaluate against data and the results obtained are almost not discussed. I suggest removing this part.

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